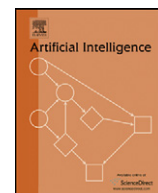


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Odd man out: Reply to reviewers

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ABSTRACT

This is the author's reply to three very different reviews of *Mind As Machine: A history of Cognitive Science* (Vols 1–2). Two of the reviews, written by Paul Thagard and Jerry Feldman, engage with the book seriously. The third, by Noam Chomsky, does not. It is a sadly unscholarly piece, guaranteed to mislead its readers about both the tone and the content of the book. It is also defamatory. The author provides appropriate responses to all three.

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I

Three very different reviews ... to which I'll reply in turn [29,47,123]. Two of them, written by Paul Thagard and Jerry Feldman, engage with my book seriously. The third, by Noam Chomsky, does not. It's a sadly unscholarly piece, guaranteed to mislead its readers about both the tone and the content of my text. It's also defamatory. But that's par for the course: I'm not the first, and I surely shan't be the last, to be intemperately vilified by Chomsky. However, more on that later.

II

I'll start with Thagard's review, because this addresses my book as a whole, rather than considering just one or two chapters. Thagard's assessment of the book in general, and of the chapters on AI in particular (and of my discussion of Chomsky, too), is highly positive. Naturally, I'm delighted. But I'll focus here on his critical remarks. And first of all, I'll address his closing questions about the future relations between AI and cognitive science.

In my book I distinguished "psychological AI" from "technological AI" (in Thagard's terms, AI as engineering): the former aims to increase our understanding of animal and/or human minds, whereas the latter does not. Technological AI—which I deliberately downplayed in the book—uses psychological (and neuroscientific) knowledge if it's helpful in achieving the task in question, but has no interest in it for its own sake. Often, it starts off by exploiting psychological research but later goes far beyond it—increasing its technological power, but decreasing its psychological relevance.

Planning is one example. The psychological experiments on human problem-solving done by Herb Simon and Allen Newell in the 1950s/1960s were highly influential in early AI—and in psychology, too (outlined in my book in 6.iii.b–c, 7.iv.b, and 10.i.b). But current planning programs, which may scale up to tens of thousands of steps, aren't inspired by psychology and nor do they interest psychologists. Another example is concept learning. As shown by the historical case-study in Chapter 13.iii.f, experimentally-based psychological theories of concept learning (due to Earl Hunt [62]) were developed by Ross Quinlan, and gradually strengthened mathematically until they were powerful enough to enable efficient data-mining in huge data-bases. Data-mining is a fine AI achievement (one of the many now hijacked by 'straight' computer scientists: see 13.vii.b), but has nothing to do with psychology.

In principle, the same sort of thing could happen again. A-Life research on epigenetic robotics, for instance, is inspired by neo-Piagetian developmental psychology and neuroscience (see below). At present, the influence of these psycho-biological

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contexts is still very clear. But that influence may be swamped by future technological advances in robotics, much as Hunt's contribution to data-mining is now all but forgotten.

Similarly, current AI work on emotions and on computer companions rests heavily on the human case [137]. New psychological discoveries will probably be exploited in future research in this area. And neuroscience too might provide helpful techniques, possibly drawing on detailed ideas about the mechanisms of learning and association in the brain. Perhaps the observable performance of computer companions will always stay as close as possible to their human inspiration. But that's simply because their usefulness, and their commercial profitability, would be compromised if they didn't. Were they to depart too far from their human model, users would perceive them as alien—maybe even threatening. Insofar as emotional intelligence is a matter of prioritizing motives and scheduling actions within complex computational architectures (7.i.d–f), decidedly un-human 'emotions' might be incorporated in some of the (non-interactive) planning programs of the future.

In short, technological AI can take psychology or leave it. Mostly, today, it leaves it.

That might change. It's clear, for instance, that many animals have a degree of computational efficiency that's the envy of AI: not for them, the frame problem! Animals and humans offer an existence proof that currently insuperable difficulties can, somehow, be dissolved. The more we can learn about that (biological) "somehow", the better AI can hope to be. And the possibility of drawing on discoveries about neural mechanisms was mentioned above.

So I agree with Thagard that "AI still has a lot to learn from other fields of cognitive science, as well as a lot to contribute." Robotics has already benefited from work in computational neuroethology, for example (14.vii, 15.vii). And the cognitive-science work which I identified (17.iii) as the most promising of all—namely, the research on complex mental architectures being done by Aaron Sloman [116,117] and Marvin Minsky [83]—could, eventually, be hugely helpful in technological AI.

Since such architectural research is a form of psychological AI, I expect it to be important also in computational psychology and neuroscience. More specialist areas too, such as computer vision or time-based neural networks (both mentioned by Feldman), might provide helpful ideas for those disciplines. But whether AI *in general* can "regain a central place in cognitive science", as Thagard hopes, is another matter. The shift towards AI as engineering is already so strong that most AI research probably won't be centrally relevant.

Now, to Thagard's criticisms. His chief complaint is that although I discussed some experimental aspects of cognitive science, I underplayed them—focusing rather on the theoretical dimension.

He's at least half right. He can even point to a signed confession. Early in the chapter on computational neuroscience, I said: "The plot of the story is concerned less with the specific discoveries that were made, fascinating though these are, than with the changes in *the sorts of question being asked* (p. 1114). So yes: whereas my earlier book devoted to computational psychology [10] had given a level of experimental detail that Thagard might well have approved, this one does not. Similarly, my previous discussion of AI [9] was packed with programming details, but this work—to Feldman's displeasure—isn't.

An easy, though nonetheless apt, defence is that the book was very long already, and one cannot mention everything. As I put it in the Preface, "Please forgive me if I haven't mentioned Squoggins! Indeed, please forgive me if I haven't mentioned someone *much* more famous than Squoggins: the characters in my narrative are numerous enough already".

I now think that I should have included a few more characters in the plot (see below). But, despite Thagard's and Feldman's remarks, I don't think I should have increased the level of detail.

In part, that's because I wanted to show a very diverse readership how AI concepts have influenced our way of thinking about the mind (or mind/brain) in several disciplines. My non-psychologist readers would have limited interest in the experimental specifics. Similarly, non-AI readers wouldn't benefit from clouds of programming technicalities. Readers innocent of neuroscience wouldn't welcome details of the biochemistry of memory, and (*pace* Chomsky, who also complained about lack of empirical detail) non-linguists wouldn't savour highly abstract minutiae about particular grammatical constructions.

However, this isn't merely a matter of interdisciplinarity. Even a single-discipline history wouldn't be focussed on nitty-gritty details and/or new empirical data. What's required instead is critical comparison and synthesis. Discussion of technicalities is often needed, to be sure, but usually (as in my book) only if couched at a relatively high level.

Moreover, one's expectations about books in this area will depend on whether one believes that all of cognitive psychology falls within cognitive science. If it does, then certain missing topics should ideally have been discussed. But if not, not.

Thagard appears to think that cognitive science does include the whole of cognitive psychology. I say this partly because he takes me to task for not expanding on Wilhelm Wundt's foundation of laboratory psychology (mentioned very briefly on p. 128), for not considering Robert Sternberg's introduction of reaction-time experiments, and for saying only a little about brain-scanning technologies such as fMRI (14.x.c). But I say it also because the title of the recent textbook he recommends, although written by two authors whom I'd be happy to call cognitive scientists, identifies its topic as cognitive psychology [120].

In my understanding of the terms, cognitive science in general, and computational psychology in particular, employ theories whose substantive concepts are drawn from one of the two machine-based "footpaths" defined in Chapter 1.ii.b: cybernetic-dynamical and formal-computational. (Or, sometimes, from both: 12.ix.b and 13.iii.c, and see [37,38,88,106].) By contrast, cognitive psychology encompasses *any* scientific study of cognition, whether machine-influenced or not. It follows that some research on cognition, howsoever influential and productive it may be, does not fall within computational psychology.

Most reaction-time experiments are a case in point. If the predicted difference in reaction-time is based on a computational theory, that's another matter. But it's the theory, not the technique, which brings the experiment within the remit of

cognitive science. Similarly, fMRI research exemplifies cognitive science only if it is prompted by (or if, *post hoc*, its results can be fitted into) some specific hypothesis about the information processing involved. As I argued in Chapter 14.x.c, that's not true of all brain-scanning work. Most of it consists of atheoretical fishing-expeditions, where no serious psychological theory, computational or otherwise, is involved. There are exceptions, of course. These include the work of Chris Frith's group on autism, schizophrenia, and tickling [7,8,16,52,113], and ongoing (unpublished) fMRI research on the theory of hypnotic control which I outlined in Chapter 7.i.h. The more that such exceptions become the rule, the better fMRI will fit into cognitive science.

There are three reasons why someone might confuse cognitive science and/or computational psychology with cognitive psychology. The first is trivial: the word "cognitive" is shared. The second is that all these terms were coined at much the same time, and often used in the same context; indeed, "cognitive psychology" was first named in a book [85] that relied heavily on computer modelling (6.v.b). The third reason is more important, because it often leads to a misunderstanding of what cognitive science aims to do (see 1.ii.a).

Many people think that something called "cognitive science" must, obviously, be the science of cognition. And in fact, most of the cognitive-science research that's been done so far (like *all* research in what's properly termed "cognitive psychology") fits under that label. But in the early days, motivation, emotion, and even personality were also regarded as key topics in their own right [131]. Moreover, when George Miller and Jerome Bruner named Harvard's pioneering Center for Cognitive Studies, they had no intention of excluding volition and emotion. The word "cognitive", in their mouths, really meant mentalist (as opposed to behaviorist)—but "mental psychology", they felt, would be "terribly redundant" [81]. Thagard himself would presumably reject a definition of cognitive science as being concerned only with knowledge narrowly defined, for—besides his seminal work in the modelling of thinking and explanation—he has recently offered a computational analysis of emotional intelligence [122].

Much the same applies to the term "artificial intelligence". This is often taken to mean that AI is concerned only with intelligence, where that word in turn is understood in a relatively narrow sense. The notion that there may be such a thing as emotional intelligence, or that AI might be concerned also with motivation, affect, and social interaction (e.g. bargaining and cooperation: 13.iii.d–e) comes as a surprise to many people. However, as the general public becomes more aware of the burgeoning research on computer companions, this particular misunderstanding may fade.

These observations link up with Thagard's statement that we need some Grand Unifying Theory of mental computation, one that would cover cognition, motivation, and affect alike. I concur (and I'd add motor control). As remarked above, what I see as the most promising/important current research is work on mental architectures. There, the aim is to provide what Thagard requests, namely "an integrated picture of how mental mechanisms operate at many different levels".

This is a tall order, and we're still a very long way off. Unlike Newell's SOAR (an early attempt to model cognition as a whole), the systems being outlined by Minsky and Sloman can't yet be implemented—although provocative mini-models of some aspects do exist [117,138]. Thagard is betting on Bayes' theorem and fMRI—among other things, no doubt. My own view is that neither of these approaches can be fully exploited until we have a much better sense of the *computational* principles and mechanisms that constitute the rich architectural complexity of the human mind.

My reservations about fMRI work (see above) were spelt out in my book (14.x.b). But my reservations about Bayes' theorem were not. In fact, I didn't mention it at all. Thagard makes no complaint about that. Perhaps he feels that it's not yet sufficiently well-established in the field to be a 'must' in a history. But I suspect that some people might nominate Thomas Bayes (1702–1761), or anyway one of his late-twentieth century disciples, as a wrongly-neglected Squoggins.

Bayes' account of probability is a latecomer to cognitive psychology (sic), but is becoming increasingly prominent there [17,89]. Indeed, a recent special issue of *Trends in Cognitive Sciences* was devoted to it [18]. Thagard is not the only one who sees Bayesian psychology as promising a Grand Unifying Theory. Others regard it as "a rigorous and coherent research paradigm that may unify the cognitive sciences, from the study of single neurons in the brain to the study of high level cognitive processes and distributed cognition" [84, p. 690]. It has been described as a "quiet probabilistic revolution" [14, p. 189], and even as "the most exciting and revolutionary paradigm to hit the cognitive sciences since connectionism" [84][p. 691]. Moreover, it is presumably what Feldman has in mind when, in closing his review, he identifies "the current great hope" as "the 'New AI' based on statistical learning theory".

In light of such plaudits, why did I decide not to discuss this approach? In a nutshell, because it usually ignores questions about psychological *mechanisms*. In terms of the distinction made above, it's part of cognitive psychology rather than part of cognitive science.

Bayesian psychologists often commend it for this very reason. They praise it as being "functional", rather than "structural" or "mechanistic", and as offering a way to theorize at all levels of cognitive science while avoiding "endless debates" about issues such as the value of symbolic versus connectionist models [84, p. 690f.]. In general, it seeks to explain why an organism decides to attend to, or employ, one type of data or procedure rather than another, without asking how those data/procedures are generated as alternatives in the first place. (It's no accident that Bayesian networks are widely used in technological AI, to aid decisions—in any area—where many different factors are *already* known to have some influence.)

To be sure, Bayesian psychology does often employ general principles of "rational induction", which go some way toward explaining where the possible alternatives come from. These principles are inspired by Roger Shepard's theory of perceptual generalization [110,111], mentioned in Chapter 7.v.a. Shepard posited various perceptual-cognitive "universals", reflecting adaptively important environmental regularities. Such cognitive universals are supposed to provide the organism's

knowledge of possible consequences—which knowledge must be specified in order to apply Bayes' concept of (subjective) probability.

But Bayesian psychologists rarely consider nitty-gritty processing. (The exceptions include Shepard himself, who has developed computer models of generalization via cognitive universals [112].) They've recently been criticized for this very reason by Gerd Gigerenzer [14], who suggests a fusion of Bayesianism with his own "algorithmic" work on "fast and frugal" adaptive heuristics (described at length in 7.iv.g–h).

As Gigerenzer says, "If the grand prize in cognitive science is uncovering both why minds do what they do and how they do it, then the productivity and scope of the metaphor [of the probabilistic mind] would ideally extend to the process level" [14, p. 189]. It's because this extension, as yet, is rarely even looked for that I omitted Bayesianism from my book. (Admittedly, I had a secondary reason too: the notorious difficulty of Bayes' theorem. An "excruciatingly gentle introduction" to it opens with a hilarious description of the bewilderment that it so commonly arouses [139].)

Given the currently burgeoning popularity of Bayesian psychology, I now regret not having mentioned it, if only briefly, when discussing Shepard's intellectual legacy. (I'd have avoided the technical minefields of Bayesian networks in AI, referring my readers to that gentle introduction, and to a more psychologically-oriented primer [39], for the mathematics.) However, and despite Thagard's high hopes, its lack of attention to mechanisms would have prevented me from listing it in my final chapter as among the most "promising" research (17.iii). In other words, Bayes can be counted as a regrettably-overlooked Squoggin—but only just.

If most Bayesians are happy to escape "endless debates" about differing computational methods, such debates can't be entirely avoided. I'm convinced—and I doubt that Thagard would disagree—that both symbolic and connectionist AI (and situationist insights, too) will be needed if and when we understand mental architecture as a whole (12.x.b). Even some technological projects, such as RoboCup, will have to combine these approaches (11.iii.b, 13.iii.c).

Moreover, it's very likely that new forms of computation will eventually be required as well. What these "new forms" will be is unknown. But, as I argued in my philosophy chapter (16.ix), we should resist the widespread notion that we know exactly what computation is because Alan Turing told us. Whether one wants to go as far as joining Brian Cantwell Smith [119] in his provocative, and often infuriating, analysis is another matter (16.ix.e). But "computation" has been, and still is, a moving target for AI researchers—and it should be so regarded by philosophers, as well.

The second main criticism made by Thagard is that I paid inordinate attention to both Continental philosophy and A-Life. And here, too, he's at least half right. Or anyway, he's half right as concerns Continental philosophy.

What I mean by that is that I would have ignored Continental philosophy if I could, for I have very little sympathy with it. However, having little sympathy with it isn't the same as thinking that it's had no historical influence, nor even that it has no valuable insights to offer.

Still less is it the same as having a knockdown argument against it. The strongest objection is that it's highly implausible, because the dramatic successes of science are most readily intelligible if science is interpreted as a realist enterprise (1.iii.b). But that argument doesn't convince the Continentals. They lie on the other side of the deepest divide in Western philosophy (see 1.iii.b–d and 16.vi–viii). This divide concerns the possibility or impossibility of a naturalistic understanding of meaning (intentionality), consciousness, reason, truth, and objectivity.

Continental philosophers argue that these notions are constructed by human minds, so the idea that they themselves could be explained by a naturalistic psychology and/or neuroscience is absurd. Technological AI, on this view, might sometimes be very useful—although necessarily limited. (Rene Descartes said as much nearly 400 years ago: 2.iii.c.) But cognitive science is doomed to defeat: not mere incompleteness, but utter vacuity.

That view isn't confined to the zanier post-modernists, or to the rampant social constructivists in the Science Wars (i.iii.b). It's promulgated also by some highly influential analytically-trained philosophers [77]. And it's popular, especially within the so-called counter-culture [104]. Alongside mistrust about comparing minds to computers, it underlies the scepticism about cognitive science that's felt by many members of the general public.

To make matters worse, this type of philosophy is increasingly being spread by people within cognitive science—most of whom don't appear to realise its full implications. Researchers whose explanations appeal to embodiment, situatedness, or autopoiesis, and/or who (like the field's early critic, Hubert Dreyfus [43]) frequently cite phenomenologists such as Martin Heidegger and Maurice Merleau-Ponty, risk being singled by this form of philosophical fire [11,34,42,79,136].

Nevertheless, some workers in this area have produced intriguing experimental results, and have offered theoretical explanations very different from the orthodox type. The psychologist Esther Thelen, for instance, has illuminated the development of rhythmic patterns such as walking, kicking, rocking, and waving, and the sudden shift between crawling/toddling, suckling/ingesting, and sleep/waking [124–126].

In addition, she has cast new light on Jean Piaget's "A-not-B error", in which infants search for a toy in its original place even though they've seen it being moved (see 5.ii.c and 14.ix.b) [127]. Whereas Piaget explained this in cognitive terms, speaking of the developing "object concept", Thelen analyses perseverative reaching by differential equations representing the interacting constraints involved [127]. These constraints include alternative movement-suites such as reaching and looking; the distance between object and child; the perceptual salience of the hiding-place; the time passed before the child is allowed to reach for it; the attractiveness of the object; and the past history of the child's actions in respect of it. Each of these had previously been shown to affect whether the child succeeded (which is a puzzle, if some object concept 'inside the head' is supposed to be responsible). But Thelen united them in a coherent way.

For all those reasons, a thorough consideration of cognitive science cannot avoid multiple references to Continental philosophy. That's true even though—i.m.h.o, as they say in the e-mails—it's highly implausible, and even though, as Thagard says, it has provided “scant theoretical or experimental contributions to how the mind works”. (As the example of Thelen shows, “scant” doesn't mean “zero”.)

The case for paying less attention to A-Life, as Thagard would have preferred me to do, is even weaker. In part, that's because many A-Life workers share the preference for Continental philosophy discussed above: if that is relevant, then A-Life is relevant too. One pertinent example here is Ezequiel di Paolo, whose intriguing simulations of perceptual homeostasis (wherein a robot can recover spontaneously from inversion of its visual field), and of the development of communication and cooperation without any information being transmitted from one agent to another, are highly unorthodox when compared with AI—or, indeed, with most A-Life [40,41]. But there are other reasons as well.

It's true that A-Life has contributed little to our understanding of the ‘higher’ aspects of the mind—not least because A-Life researchers deliberately avoid talking about them. Insects are considered challenging enough. Moreover, internal representations of various kinds—which A-Life eschews—are, I believe, necessary for certain types of behavior (13.iii.c, 14.viii). These include not only human-level conceptual thought but also motor control (14.viii.b), and spatial cognition and planning in many animal species [66,115,118]. However, Simon's famous—and thoroughly *situated*—ant [114], and the closely-related technology of production systems [87], offered novel and productive ways of thinking about some aspects of human thought and behavior. Similarly, situated robotics and more biologically oriented areas in A-Life (such as computational neuroethology: 14.vii and 15.vii) may offer suggestive ideas about some aspects of functioning in human minds.

There are historical reasons, too, for discussing A-Life at some length. It grew out of mid-century cybernetics (thanks to John von Neumann, Grey Walter, and Ross Ashby: 15.v, 4.viii) and Turing's [134] work on morphogenesis (15.iv). In other words, it was originated by the same group of people who originated both symbolic and connectionist AI. To be sure, most individual members had a preference either for what we would now call A-Life (bottom-up) methods or for top-down GOF AI (Good Old-Fashioned AI) approaches. But at that time they were friendly colleagues in the quest, agreeing to differ but sharing largely similar goals. The sociological schism came later (4.ix). In my view, then, A-Life counts as a form of AI, not as something utterly distinct from it. (Rodney Brooks himself, in his early manifesto [15], was ambivalent about whether or not situated robotics counts as “AI”: see 1.iii.g.)

Lastly, there are philosophical reasons for including A-Life within cognitive science. If, as is often said but very rarely explicitly argued, mind can arise only in living things, then A-Life is theoretically—and perhaps even technologically—prior to a successful AI and/or computational psychology (16.x). More specifically, many workers in A-Life are sympathetic to autopoietic biology (a special case of the Continental approach), which they see as an explanation for much/all of the self-organization observed in living things [79]. If anything like this is correct, then A-Life is not merely a form of (“strong”) AI, but part of its foundations.

Whereas Thagard thinks I overestimated the importance of Continental philosophy and A-Life, he thinks I underestimated that of cognitive linguistics. And here, he is 100.

I merely touched on it in the linguistics chapter (9.ix.g). (I'd realised that it deserved more attention, but the chapter was so long already that I decided—wrongly—not to add further details.) Elsewhere, I considered one example at length: Daniel Sperber and Deirdre Wilson's work on relevance [121]. This was crucial in my discussions of the explanation of individual thoughts (7.iii.d), and of the cultural spread of certain religious beliefs rather than others (8.vi.e). And cognitive linguistics figured also in the subsection on situation semantics and embodiment (12.x.g)—which concerned an ambitious research project briefly referred to below [35]. However, I should have said more.

The particular cognitive-linguistics Squogginsees recommended by Thagard are George Lakoff (mentioned also by Feldman, one of Lakoff's research collaborators), Ronald Langacker, and Leonard Talmy. Of those three, I'd focus primarily on Lakoff [70–72]—although this would increase the space given to Continental philosophy, so Thagard wouldn't be entirely happy. His recent book (co-authored with Mark Johnson), in particular, is a rich source of ideas on the metaphorical and bodily basis of language and cognition [72].

Lakoff and Johnson argue there that a sensorimotor base underlies all our categories, including abstract philosophical concepts such as cause, number, mind, and morality. And they deny that concepts are strictly definable in terms of necessary and sufficient conditions. On the one hand, concepts allow both for borderline cases and for some instances having a more ‘central’ status than others: see my discussion of family-resemblances in 9.x.d, and of prototypes in 8.i.b. (The latter example, by the way, belies Feldman's suggestion that I ignored prototype effects.) On the other hand, they are often ill-defined in the sense that they are based on more than one bodily metaphor. “Cause”, for instance, involves nearly two dozen different metaphors, carrying somewhat different inferential patterns (e.g. forces, pathways, links, correlations ...).

This is bad news for a logicist AI, or a logicist philosophy, that tries to shoehorn concepts/cognition into the unforgiving forms of symbolic logic. Naïve physics, for example, can't be formalised in terms of the predicate calculus (a conclusion that Patrick Hayes himself has now drawn: 13.i.b).

Thagard is also right about there being too little on recent cross-cultural psychology (for a survey, see [80]). And I endorse both of his two Squogginsees here. Douglas Medin's work (with the anthropologist Scott Atran) on folk-biological categorization and ecological reasoning, in traditional and industrial cultures (drawn from North and Central America), goes far beyond the purely classificatory folk-biology research mentioned in my anthropology chapter (8.i.b and 8.i.d) [3,80]. And Richard Nesbitt's comparisons between the thought-patterns, and cultural conventions, of East Asians and European

Americans show some systematic differences that lie deep in people's minds [86]. Asian thinkers, he claims, are less wedded to "logical" classification and thinking, and more "holistic" and "dialectic" (i.e. harmony-seeking), than Westerners are.

I'd even add an extra Squoggins at this point, namely the classical historian and sinologist Geoffrey Lloyd, who has recently placed these experimental data in the context of his deep scholarly knowledge of Western and Eastern cultures [76]. Among other things, he shows that Nesbitt's account of Eastern/Western cognitive differences is overly simple: Aristotle, for example, was the father of Western logic (the syllogism), but also insisted that good reasoning in practical matters requires virtue as well as mere formalistic cleverness. (One might also add that some distinguished Western logicians have questioned one or more of the four classical laws of logic.)

Cognitive scientists intrigued by the specific phenomena, and the detailed experiments, reported by Medin and Nesbitt and discussed by Lloyd will want to consider just what computational processes might underlie them. Relevant areas of investigation here include emotional intelligence (7.i.d), fast and frugal heuristics [54,55] (7.iv.g), and the computational benefits of bounded rationality in general (7.iv.h). ("Benefits", because bounded rationality is not simply an annoying limitation on our mental powers: given that it has evolved in certain ways, it is actually an advantage [59].)

I'm grateful to Thagard for his interesting correction concerning the origin of "bug". On googling the term as a result, I discovered that Thomas Edison had used it in his notebooks since the 1870s (see www.worldwidewords.org/qa). His word simply meant "a problem": bugs were *imaginary* insects, much like the "gremlins" routinely blamed by British engineers—and Royal Air Force pilots—in the 1940s/1950s. (I remember that term being very widely used, in everyday chat about mechanical failures and in philosophical arguments too.) There's a suggestion, however, that even Edison didn't originate it. Apparently, telegraphers were already using the word (literally), suggesting that electrical faults were caused by real bugs getting into the cables. In that case, Grace Hopper's famously photographed insect was a twentieth-century cousin of those nineteenth-century pests.

Before leaving Thagard's piece, I hope I may be allowed one niggler. He says that "post-Piagetian developmental psychology gets short shrift". Yet I devoted seventeen pages to it in the psychology chapter (7.vi.f–i) and twelve in the neuroscience chapter (14.ix.b–d). I also referred to it, and to the key concept of epigenesis, in the A-Life chapter (15.iii.b, iv.b, and viii.a–b). This topic is important not least because it has helped to provide a much more subtle, and much more plausible, notion of *nativism* than was previously employed by psychologists—Piaget excepted (6.ii.c). Today, other cognitive scientists too are using this approach: as remarked above, epigenetic robotics (of which MIT's "Cog" robot was an early example) is a case in point.

III

Feldman, too, makes some highly complimentary remarks about my book. I'm especially pleased that it gave him a new perspective on events that he himself had been involved in. Overall, however, he likes it less than Thagard does.

Although he'd welcome more technical detail on AI (see above), he feels that there's too much material in the book as a whole—which is why he focussed on only two chapters. I grant him that the book is very long. And I salute his witty choice of the epigram from George Bernard Shaw: "What else he says I do not know; for it is all in a long poem which neither I nor anyone else ever succeeded in wading through" (but I'm tempted to remind him that Shaw put these words into the mouth of the Devil!). Swapping quote for quote: the poet Stephane Mallarme famously said "Everything in the world exists to be put in a book". A history of the highly interdisciplinary field of cognitive science needn't mention everything in the world, to be sure. It will even have to omit a host of marginally relevant Squogginses. But if it's not to be superficial, it can't avoid being both very long and very thematically diverse. (All the more so, if it tries to situate the scientific developments within their sociopolitical context: see 1.iii.b–d, 2.ii.b–c, 3.v.d, 4.vii.a, 11.i, 11.ii.f–g, 11.v.)

That point is related to Feldman's comment—not, he says, a criticism—that the book I was trying to write is in principle unwritable. Well, yes and no.

A wholly objective, fully comprehensive, and 100indeed impossible. So I wasn't aiming to square that particular circle. Any history (as I said in opening the Preface) is a narrative written by a particular individual, for particular purposes, and from a particular point of view. Different authors will stress different themes. Even if the very same topics are discussed, each writer's account of them will be somehow different. For instance, different Squogginses will be left unmentioned; and different potential applications (to psychopathology, for example) will be stressed. Hence Feldman's apt title: "Her Story of Cognitive Science".

To compensate for this as far as possible, my Preface indicated how my own intellectual background and priorities would inform the text as a whole. Besides the mind-body problem and mental evolution in general, I've always been intrigued in particular by purpose, freewill, creativity, and psychopathology. Moreover, my own education has been highly interdisciplinary. So (as I put it) the future I was trying to legitimize, in giving my particular story of the past, is one in which interdisciplinarity is valued and alternative theoretical approaches respected—and, so far as possible, integrated. I hoped that specialists in each discipline would discover some 'disciplinary' historical facts that they hadn't known before (so I was heartened by Feldman's comment that I did "an outstanding job of promoting some of the forgotten contributors"). But, even more important, I hoped that they would become aware of a host of historical and contemporary connections between their own speciality/discipline and others.

As regards Feldman's criticisms under the 'Squoggins' heading, he identifies the key absentees from my story (besides Lakoff, as remarked above) as the neuroscientists Richard Thompson, Moshe Abeles, and Leon Cooper.

He recommends the latter two when rebutting my claims about the relative lack of AI-modelling work on neural timing (14.ix.g). But Feldman himself admits that their work is highly “technical”. Moreover, it has limited interest for cognitive science. Conceivably, that might change: these neural details may become more widely relevant, even (as suggested above) for furthering technological AI. At present, however, they are of interest primarily in highly specialist neuroscience. So, given my avoidance of nitty-gritty technicalities in general, I stand by my omission of Abeles and Cooper.

Thompson is another matter. He was the first person to be appointed to Karl Lashley’s chair of Psychology at Harvard after Lashley’s death in 1958. That’s ironic, because he is widely known today for his work on memory localisation, a notion which Lashley’s experiments in the 1920s seemed to have disproved definitively (5.iv.a).

Thompson’s influence began with his mid-1980s discovery that learnt reflexes, such as conditioned eye-blinks, are stored in the cerebellum [128]. Now, and partly thanks to Thompson [67,129,130], it’s known that other kinds of memory are located in other parts of the brain. For instance, episodic memories of particular experiences seem to depend on the hippocampus and the temporal lobe (whose involvement was first suggested in the 1950s, by Wilder Penfield’s experiments on conscious patients undergoing surgery for epilepsy: 14.x.a).

Besides offering much detailed information about memory location, modern neuroscience can say something about the neuronal (and biochemical) mechanisms of memory formation. Thompson has contributed hugely to that latter topic, too. Moreover, his ideas have inspired some neuronally specific simulations that are worthy successors to David Marr’s pioneering cerebellar models, discussed in Chapter 14.v.c–d. In short, I accept that I should have featured him (briefly) in my book.

The paradox of the apparent contradiction between the adjacent holders of that Harvard chair can be eased by noting the ambiguity of “localisation”. In my discussion of the swinging pendulum of popularity of grandmother cells (14.ix.e.), I cited Mike Page’s recent “Localist Manifesto” [90]. This argues not only that some degree of localisation is essential, but also that localisation and distribution can be identified on various levels, so aren’t mutually exclusive. As Page puts it: “Localist models are characterized by the presence of localist representations rather than the absence of distributed representations”. (I compared this with Minsky’s K-line theory, which combined single-unit significance and distributed processing: 12.iii.d.) Disputes about grandmother cells are focussed at a greater level of detail than Thompson’s 1980s work, but are relevant to much of his later research.

Most of Feldman’s critical remarks relate to connectionist AI. One of these puzzles me deeply. Indeed, I can’t imagine what led him to complain (p. 1108) that “the core PDP party line, which is not mentioned in this chapter [on connectionism], was that networks were plausibility demonstrations against requisite innateness”.

This anti-Chomskyan position was indeed their core claim—or rather, a highly visible part of it. Their fundamental challenge to the GOFAI orthodoxy was to deny the psychological reality (and computational necessity) of formal processing rules—of which grammatical rules, whether innate or not, are a special case. Thus far, then, Feldman is right.

But his other claim here is simply false: Chapter 12, on connectionism, discussed the PDP party line at length. First, in a subsection called “Wonders of the past tense” (12.vi.e), which dealt with the theoretical dynamite provided by the original PDP past-tense learner [105]. Again, in the section describing philosophers’ responses to connectionism, both for and against (12.x, and especially 12.x.d). And yet again, when discussing later PDP work on recurrent networks (12.viii.b) and the role of input history (12.viii.e). In addition, I gave cross-references to my discussions of Chomsky’s nativism elsewhere, especially in 7.vi.g, which outlined the “third-way” form of nativism: namely, epigenesis (favoured by the PDP researchers Jeffrey Elman and Kim Plunkett, among others [45]).

The first PDP past-tense learner was “theoretical dynamite” because it undermined Chomsky’s claim—accepted by most pre-1980s cognitive scientists (see below)—that children’s acquisition of language can be explained only by their applying formal rules of grammar. In particular, it challenged his argument that the commonly observed infantile errors relating to irregular forms (such as saying *goed* instead of *went*, and *mouses* instead of *mice*) couldn’t possibly be explained by learning, because the child never actually hears these mistaken words spoken. Chomsky explained them as over-regularisations of formal-compositional rules, such as *Form the past tense by adding -ed to the root* and *Form the plural noun by adding -s to the singular form*. These particular rules clearly aren’t innate, for they don’t apply to all languages. (So, *pace* Feldman, the past-tense learner didn’t challenge nativism directly.) But Chomsky believed them to be closely dependent on innate syntactic rules, namely, the (unknown) universal grammar: 9.vii.c–d. In short, *that there are formal rules of grammar* (some innate and some not) was taken by Chomskians to be proven by these “over-regularising” mistakes.

As Feldman says, PDP provided a “plausibility demonstration” which suggested a very different—i.e. statistical—explanation. The past-tense learner showed transitory “over-regularisations” following initially error-free performance. In other words, it temporarily settled on *goed* instead of *went*, even though it had previously got this verb right. The same is true of children. And, crucially, the network’s verbal input over time was statistically similar to that experienced by them. Most of the verbs heard by very young infants are irregular (*go, come, give, take, get, be*); likewise, the network’s earliest input contained only ten verbs, of which eight were irregular. Later, these get ‘swamped’ by the addition of scores of regular verbs (for the network, an additional 140). The correct irregular forms are eventually relearned, because they remain as commonly-used items in the input.

This match in the overall contour of child/network behavior generated an explosion of interest across cognitive science. However, the theoretical stakes were very high. The new party line was pitting statistics against formalist orthodoxy, PDP against Chomsky. Accordingly, “overall” wasn’t good enough, and there was huge controversy over the small print (12.x.d). Classical computationalists pointed out that the *details* of past-tense acquisition differed as between child and network [94]

(and also offered principled arguments about the limits of connectionist computation: see below.) But the PDP workers persisted, and later achieved a closer child/network match: 12.viii.c [95,96].

Although the PDP party line had provided a fearsome challenge to the classical approach, it hadn't (yet?) won the battle. Much of the controversy concerned the plausibility of that little word, *yet*. The key problem was that a PDP network, because it settles into *global* equilibrium, can represent only one thing at a time.

Feldman accuses me of "ignoring" this, so of not having "recovered from drinking the Kool Aid" of PDP and Hopfield hype. To the contrary, I mentioned this fact in relation (for instance) to attempts to build "Assemblies of cell-assemblies" (12.x.a). And the long section on philosophical responses to PDP (which perhaps he did not read?) clearly showed that distributed representation is highly problematic. The difficulty, in a nutshell, is that sentences in natural language, and logical formulae too, have a hierarchical structure. This structure is crucial for interpreting any 'stand-alone' proposition, and also for reasoning from one to another. (N.B: *reasoning from*, not *associating with*: see below.)

GOFAI had drawn both its 1940s promise and its ensuing successes from logics defining ways of reasoning with hierarchical formulae: primarily, the propositional and predicate calculi, plus Emil Post's production systems (see 4.iii.c–e, 10.v). Indeed, the logical/symbolic 'side' of cybernetics had burgeoned with the appearance of digital computers precisely because it seemed able to represent semantic meanings and reasoning, whereas the adaptive/dynamical 'side' did not (4.ix).

Naturally, the post-1980 PDP experts hoped to recover the lost strengths of GOFAI: in a word, reasoning. To this end, they tried to build hierarchical representations—including family-trees, syntactically structured sentences (alias propositions: see below), and complex networks of networks (12. viii.b–c and ix.a). Similarly, the philosophical champions of PDP sought to show that, *in principle*, compositionality—and what Gareth Evans [46] had called the generality constraint—can be achieved by distributed systems [31,32]. Neo-Chomskyan philosophers, meanwhile, vehemently denied this [51].

My connectionism chapter discussed all these efforts to implement PDP-hierarchy (12.viii–x). So Feldman is wrong when he says that I claimed that the PDP Group "didn't even try to model propositions". If he were to re-check the supposed quotation from my book (p. 964), he would find that what I actually said there was that they didn't try to model *propositional reasoning*.

Their failure to model propositions (sentences), which I described, meant that they could model reasoning only in terms of conceptual associations. These are hugely important in human thinking, to be sure. (They are a key notion in the Sperber–Wilson theory of relevance, for instance: see above.) But the rigorous reasoning that can be achieved by GOFAI (in planning, for example, or expert systems) was so far beyond them that it wasn't even attempted. As for their efforts to represent hierarchies (propositions), these were so discouraging that—as Feldman puts it—they eventually gave up.

In other words, what I called the "schism" between the two sides of cybernetics persisted. If the adaptive/dynamical side is represented only by PDP, the schism is doomed to persist indefinitely. For, *pace* some philosophers [31,32], global distribution (as Feldman rightly says) can't implement compositionality.

Similarly, "dynamical systems" whose designers claim to be modelling problem-solving, the jewel in GOFAI's crown, do no such thing (16.vii.c). Admittedly, they can simulate the continual shifts of attention to, and confidence in, the various considerations relevant to a complex decision [132]. In that sense, they can model important aspects of some sorts of problem-solving: those where the decision-making depends on associative and/or emotional mechanisms. But rigorous sequential reasoning depends, rather, on structural comparisons and manipulations.

Moreover, dynamical systems theory shares a weakness with the Bayesian approach that was remarked above. The various "considerations" involved in the decision-making are provided by the programmer for free, whereas in real thinking they have to be generated by the system itself (see 14.ix.b, 15.viii.c, 16.vii.c). According to Shepard, they are provided by, or anyway grounded in, inherited expectations about the environment. Thelen's dynamical accounts of behavioral phase-changes such as gait-switching, and of the A-not-B error, similarly assume that evolution and development (plus learning) will have provided the "constraints" which—so she claims—mold the infant's behavior. Her differential equations shuffle the constraints in a very interesting way, but say nothing about just how they arose.

The cybernetic schism will be productively crossed only with the development of hybrid systems. My book outlines some existing examples (see 12.ix.b). But it also features an exciting project for the future, wherein a *rapprochement* between the two sides is being attempted from a richly interdisciplinary standpoint (12.x.g) [35]. The research is being headed by Andy Clark, who is well aware of the importance of both compositionality and association [31–33] (and who is a leader in the philosophy of embodiment [34]). *A propos* Feldman's complaint rebutted above, it's worth remarking that I introduced this project as follows: "I said [above] that the late-century connectionists didn't try to tackle propositional reasoning. However, Clark (2005) has very recently outlined a research programme aimed at doing so—and at taking *embodiment* seriously in the process."

Another puzzling claim in Feldman's review is that "You will find little about AI developments after 1980". As explained in reply to Thagard, I ignored purely technological AI—except for the case-study of machine learning. So a high proportion of post-1980 AI advances were ruled out of consideration. But psychologically relevant work was considered at length.

Besides the 30+ pages on efforts to strengthen PDP (12.viii–x), the whole of Chapter 13 describes post-1980 GOFAI-based research. Two sections (13.v–vi) concern HCI, virtual reality, and generative computer art—which Feldman may not count as AI. But the others discuss center-field topics such as logicism, expert systems, case-based reasoning, learning, planning, agents, and creativity. Post-1980 AI also features prominently in other chapters: for example, in accounts of natural language processing (NLP), mental architecture, emotion, reasoning, vision, distributed cognition, and robotics. (Feldman

reports “little” on robotics too, but psychologically relevant examples are described in the chapters on neuroscience and AI-Life: 14.vii; 15.vi.c–d, vii, and xi.)

If I am sometimes puzzled by Feldman, the reverse is also true. He asks (p. 1108) what we are to make of my statement that “After all, the two types of AI (connectionist and symbolic) were in principle equivalent, since both were using general-purpose computational systems”. And he adds that he can’t read it “in any way that does not suggest someone who just doesn’t get the point of computational models in Cognitive Science”.

The answer to his question turns on the words “using” and “equivalent”, both of which are equivocal. The meaning of (computational) equivalence isn’t actually relevant, because my choice of this term was a careless slip: I should have said “compatible”. (Sorry!) This conforms with what I meant by “using”. If an AI system is “using” X, this could mean that it is *implemented in* X, where X is a physical machine. Or it could mean that X is the virtual machine defined by the system, which is *being used as a model of something* (of psychological processes, for example). Here, I intended the former meaning. I was discussing hybrid systems (12.ix.b), and had pointed out that although these were recommended in the 1940s, people in the 1990s were expressing disappointment that there was still no general theory of how to build them. (Perhaps that’s impossible; for a survey that deliberately forefronts the connectionist aspects, see [135].) What I meant by my puzzling remark was that there could be no obstacle *in principle* to hybrid systems, since both GOFAI and (most) connectionist networks are implemented in general-purpose computers. *In practice*, however, the two types of virtual machine are very different (as I said), so it’s not easy to combine them effectively.

I acknowledge, then, that my choice of words here was highly unclear: *mea culpa*. But I trust that the text as a whole shows that I understand very well what is “the point of computational models in Cognitive Science”.

My final response to Feldman is to protest at his comparing my book to science journalism. His comparison is welcome insofar as it implies high accessibility (to my pleasure, he describes the book as “very well written”, and says he enjoyed reading it). But it also suggests superficiality, lack of scholarship, and even triviality.

The first of these tacit charges was answered above, where I explained why the book had to be so long, and where I pointed out that I did provide technicalities (although not at the level of detail which Feldman would have liked). The second was in effect rebutted by Thagard, who noted that there are over 5000 items in the bibliography. (For the record, these weren’t just lifted from other people’s reference-lists: every one had been read by me—usually, in full.) The third charge is more interesting, and requires an answer here.

Feldman says that the book provides “a good deal of juicy gossip about personalities and conflicts”. So it does. But this material isn’t mere trivia. In other words, it wasn’t included—as it would be in science journalism—for titillation, or simply to lighten the narrative. It’s there for a solid historiographic reason: to show that what the physicist John Ziman [140] has called “the Legend” of purely disinterested science is false (1.iii.b–d).

Personal (and sociopolitical) factors can hugely influence which ideas are favoured or ignored by the scientific community. My discussion of the Lighthill Report, for example, shows that this notorious AI *affaire* was prompted by the unusual personality of one individual, Donald Michie (11.iv and v.c). And the so-called “twenty-year famine” in research-funding for connectionism was due in part (though only in part: 12.iii.e) to a long-lasting friendship between two influential men: Minsky and Joseph Licklider. The history of cognitive science can’t be properly understood unless the Legend is specifically repudiated. That’s why I gave a wide variety of examples of decidedly non-Legendary behavior.

And that, as it happens, brings us to Chomsky.

IV

Anyone who believed in the Legend would be sadly disillusioned on reading Chomsky’s review of my Chapter 9 [29]. It’s a sorry contribution. Far from being a disinterested intellectual engagement, his essay displays more bile than logic. It systematically misinterprets and misrepresents my text, and also contains outright falsehoods—not all of which merit the charitable label of “mistake”.

Some of his claims—for instance (p. 1096), that I chide him for having changed his mind—are so inherently incredible that a cautious reader would take them with a pinch of salt. And his sustained intemperance might also engender doubts. But many non-linguist readers, unaware of Chomsky’s habitual manner of argument (of which, more below), may feel that where there’s smoke there surely must be fire. Certainly, no-one reading his splenetic attack would guess that I dwelt at length on the hugely beneficial influence that Chomsky’s early work had on cognitive science (and computer science, too), nor that I stressed his importance in raising certain core questions in linguistics.

Instead, his readers would infer that my chapter was an intellectual hatchet-job intended to destroy his reputation: in his words, a “campaign” motivated by “her rage and ridicule”. Indeed, an American urological surgeon who’d found the review on the web sent me a very funny e-mail, saying “As a urologist, I recognize a ‘pissing contest’ when I see it”.

The point, however, is that it wasn’t a pissing contest initially. There was no rage, no ridicule. My chapter wasn’t written in a polemical spirit. It wasn’t even written in a negative spirit. To the contrary, it recorded—and applauded—Chomsky’s crucial role in the founding of cognitive science. It contained some criticism, to be sure. And it quoted more (some polemical, some not). But that’s a very different matter. How could an honest intellectual history be written without recording the negative reactions too?

One reason for the web-grazing urologist’s misconception lies in Chomsky’s overly defensive misinterpretations of words that I had used neutrally—or even in praise of him. For example, he takes my section-heading “That Review!” to be an

attack, quoting it sarcastically on several occasions. In fact, it was saluting the huge fame of his review of Burrhus Skinner [20]—which many of my non-linguist readers would already have heard of, even if they hadn't read it. Similarly, he misreads my reference to his general political disagreement with Skinner. He says that I claimed to find political content in his review of Skinner's *Verbal Behavior* (which I did not), and that like charges are "repeated throughout" my chapter (also false). What's more, he assumes that my remark was intended as criticism—even though it occurred in the context of my commending his bravery in the political sphere, comparable (I said) to that of Bertrand Russell, who suffered prison on more than one occasion as a result of his political views (p. 641).

The key reason why the American MD was misled, however, lies in Chomsky's unscholarly strategy of continually quoting contemptuous terms picked out of my text as though I had used them myself. I had not. Rather, I had quoted them—from critiques of Chomsky written by professional linguists and psycholinguists. His inability to see the difference leads to misinterpretation over and over again. It follows from the elementary distinction between *mention* and *use* that quotation doesn't necessarily imply agreement. In particular, the barrage of negative terms with which he opens his review contains many that weren't endorsed by me.

In most cases, I didn't endorse them because I didn't share the emotion expressed by them. (See my remarks, below, on the unfortunate within-discipline effects of Chomsky's rhetorical style.) In some cases, however, I pointed out that I was in no position to endorse them. I said, for instance, that I lack the mathematical skills that would enable me to vouch for the judgments (quoted on pp. 651 and 654) that Chomsky's argument in *The Logical Structure of Linguistic Theory* was often "maladroit" and "perverse", and sometimes "just plain wrong" [107, p. 366], or that Chomskyans often state "purported 'theorems' ... without any proof being suggested, or theorems that are given 'proofs' that involve no definition of the underlying class of grammars and are thus empty" [53, p. 14].

Notice, however, that (as I reported) the two internationally distinguished linguists quoted here had assured me that they meant exactly what they said. I'd pressed them hard on this matter because, having previously accepted the widely-held belief that he always achieved mathematical rigor in his work (see below), I'd found their claims highly surprising. This undermines Chomsky's charge that "Her conception of an argument, repeatedly, is to quote someone who agrees with her judgments. QED": in some cases, I had no such prior judgments, being agnostic on the matters concerned.

So why, you may ask, did I mention the adverse judgments of others? Why not emulate Thumper's mother, who famously advised her baby rabbit that if he couldn't say anything nice then he shouldn't say anything at all? Or, if I had to report that some people disagreed with him, why also cite their less-than-temperate words? Weren't these startling quotations simply academic muck-raking—alias a pissing contest?

No, not at all. Another widely-held belief (again, see below) is that his linguistic theory is "indisputably" correct in its essentials. In other words, that all serious linguists today accept his approach. The quotes referred to above show that they do not—indeed, that many reject it with an unusual degree of passion. Just why Chomsky has attracted such venomous opprobrium from his disciplinary colleagues is an interesting question, to which I'll return at the end of this *Reply*.

As he remarks, the historical and scientific issues central to my Chapter 9 arose within my discussion of what I called "the tenfold Chomsky myth" (p. 592). So it may be helpful if I list its components here:

- (1) Besides giving a *vision* of mathematical rigor, he always achieved it in his own work.
- (2) His linguistic theory was (or is now) indisputably correct in all its essentials.
- (3) The nativist psychological implications he drew from it were convincingly argued at the time, and
- (4) they are now empirically beyond doubt.
- (5) His work of the 1950s was wholly original.
- (6) His writings of the 1960s were, as he claimed, the culmination of a tradition of rationalism dating back 300 years.
- (7) Without Chomsky's grammar there would have been no computer modelling of language.
- (8) Chomsky was responsible for the demise of behaviorism.
- (9) He reawakened and strengthened the discipline of linguistics.
- (10) Linguistics is as prominent in cognitive science today as it was, thanks to Chomsky, in the late 1950s to 1970s.

Each of the laudatory beliefs in the myth, I said, is near enough to the truth to explain why it's so widely accepted. Indeed, as remarked above, I'd never questioned item-1 myself before doing the research for this chapter. Nevertheless, interpreted strictly, each is false. The least defensible is item-10: that linguistics, especially Chomskyan linguistics, is as prominent in cognitive science today as it was forty years ago. The fact that it isn't surprises many people, and cries out for explanation — which I provided (see below). The most defensible is item-9. That he reawakened and strengthened the study of syntax (and semantics) is undeniable. But that he did this for linguistics as a whole is not. In some ways, as we'll see, he set the discipline back.

Before saying anything else, I must make one important clarification. It concerns a misinterpretation on Chomsky's part that was largely my own fault. At the outset of Chapter 9, I used a phrase whose meaning I thought was clear in context but which was in fact ambiguous. And for that, I apologize.

Specifically, I contrasted those people who, because of his courageous political writings, take Chomsky as their "political guru" (making him the most widely quoted living writer, and the eighth most-quoted of all time [5, p. 3]) with "those who uncritically [sic] take him as their scientific guru". By the latter phrase, I meant people on the outskirts of cognitive science, or students having only a superficial knowledge of it. This group, I said, accept most or all items of the myth.

That they do so has indeed been my experience. However, since the people I was thinking of were outsiders and/or beginners, citations would have been inappropriate. A similar point, relevant to myth-items 2–4, was recently made by someone else: “The general public still appears to see Chomskyan linguistics and the idea of innate cognitive structures as an unchallenged consensus” [109]. Moreover, Chomsky’s arcane text *The Minimalist Program* (1995) was hailed in a British national newspaper in 1998—not in a book review, nor even a feature article, but as a quarter-page news-item. Clearly, the editor accepted, and expected his readers also to accept, item-10: the belief that Chomsky’s linguistics is as prominent in the scientific study of mind today as it was many years ago.

I now realise, however, that—despite my deliberate insertion of the word “uncritically”—the second phrase could be supposed to mean *Chomskyan linguists*. That’s especially likely if it were read—as Chomsky admits it was, by him—in the context of Chapter 9 alone, ignoring the rest of the book. However, a Chomskyan who did read some other chapters interpreted the phrase in the same way, saying “[she clearly implies] that Chomsky and his associates have somehow developed a largely illusory belief system” and he, too, complained about lack of references [102]. Had that been what I meant, it would indeed have been remiss not to give references. So the complaint that I didn’t do so is understandable.

Chomsky’s other charges of lack-of-references, however, are mostly false. Many of the 5000 bibliographic items remarked on by Thagard were cited in this chapter. And some of those attracted multiple page-references: *Syntactic Structures* (SS) [19], for example (notwithstanding Chomsky’s claim that I “scarcely looked at” it). At one point (p. 593), I omitted a name deliberately. I mentioned a conversation with a young linguist from MIT who had never heard of an important competing theorist. I didn’t identify this person: the fault wasn’t his/hers, but lay rather in the standards of scholarship surrounding them at MIT.

Although I did not ascribe belief in the tenfold myth to Chomsky himself, nor to Chomskyans in general, it seems to me to raise many key concerns of his work.

He disagrees: he complains that “the topics that have been of primary interest to me in linguistics, philosophy, and cognitive science” are alluded to “only tangentially, if at all” (p. 1094). Since he coyly omits to say what these are, it’s difficult to counter his remark. Certainly, I chose not to discuss his later theories in any detail, because (contra myth-item 10) they haven’t featured in cognitive science. But several Chomskyan topics which did affect it intimately are discussed in the chapter he read. Others, to which cross-references are given, are considered elsewhere in the book. These include his influential work on the competence/performance distinction, on nativism in psychology and the philosophy of mind, on modularity, and on internal representations (see especially 7.iii.a, 7.vi.passim, 12.x, and 14.viii).

Most of Chomsky’s complaints about my chapter relate to myth-items 2–4. These concern science more than history, although some historical points do arise. Before considering what he says about nativism (items 3 and 4), I’ll focus on what he says concerning his theory of language as such—the topic of myth-item 2.

One aspect of myth-2 has already been discussed: the mistaken notion that no serious linguist “disputes” Chomsky’s approach. The other aspect—that his linguistic theory actually was/is essentially “correct”—can’t be dealt with so quickly.

Whether (any version of) Chomsky’s theory is correct depends, as he repeatedly says in his review, on what the “empirical facts” about language actually are. He accuses me, again and again, of neither knowing nor caring about this question. But he fails to point out that the answer depends, in large part, on a tricky theoretical/methodological problem: *what sort of thing*, in principle, an empirical fact about language is. And that is something which I did address in my chapter. I said (p. 630) that, in making judgments about grammaticality, Chomsky’s reliance on the intuitions of native speakers, and especially his habit of relying primarily on his own intuitions, didn’t satisfy “data-respecting critics” in the late-1950s, and doesn’t satisfy them now. “Such a source,” I remarked, “hardly seems reliable”.

He objects vociferously, agreeing with my statement about reliability but denying that it applies to him. His self-defence is that “Boden was apparently unable to discover someone to quote” (p. 1096). Well, it’s true that I didn’t give a verbatim quotation. But I did refer to his tutor Zellig Harris’s sage advice—published in the same year as SS—on how intuitive grammaticality-judgments needed to be carefully controlled so as to avoid experimental bias of various kinds [58, sect. 4]. I didn’t add any references published long after SS, because the point seemed so obvious. So let me now satisfy Chomsky’s request for further chapter and verse.

The classic source on this question, which was largely prompted by Chomsky’s continuing failure to take Harris’ advice seriously, is the paper by the sociolinguist William Labov on “Empirical Foundations of Linguistic Theory” [69]. This was Harris’s warning with knobs on. Labov showed conclusively that the intuitive judgments of the native speaker (although useful as a first port of call [69, p. 103]) are not a reliable indication of grammaticality. In other words, they are not a proper source of linguistic facts.

Labov gave various reasons why “the uncontrolled intuitions of linguists must be looked on with grave suspicion” (p. 102). But “the most damaging body of evidence on the weakness of intuitive data”, he said (p. 104), was research showing that someone may intuitively reject a certain word-string as ungrammatical which on other occasions they are happy to use. For instance, he had observed “hundreds” of examples of a Philadelphia dialect in which *anymore* was used to mean *nowadays* (97f., 106–108). Native speakers might insist that a word-string employing *anymore* in that way is (a) never used, and (b) uninterpretable, *even though* they had been heard to use it themselves. As Labov commented (p. 107), “This puts us in the somewhat embarrassing position of knowing more about a speaker’s grammar than he does himself”.

His explanation was that the Philadelphians had been unknowingly influenced by faulty teaching in the schoolroom: their theoretical ideas about what their language is like had blinded them to the empirical realities of what it actually is like.

Academic linguists aren't immune to this type of bias. They are subject, too, to what psychologists term "the experimenter effect", wherein people unconsciously behave as the experimenter expects them to behave [103].

It followed, of course, that "Chomsky should not accept his own judgments on [any] issue in which he has an established theoretical position" (p. 101f.). Worse (from Chomsky's point of view), "linguists cannot [i.e. should not] continue to produce theory and data at the same time" [68]. And the killer: in the case of any disagreement on intuitions, "the judgments of those who are familiar with the theoretical issues may not be counted as evidence" [69, p. 103].

This was a damning indictment of Chomsky's methodology. Nevertheless, Chomsky continued to accept intuitive judgments (often his own, or those of his students) as his prime source of "linguistic facts". His occasional admissions that native speakers' intuitions are not "sacrosanct and beyond any conceivable doubt," and that "their correctness can be challenged and supported in many ways ..." [22, p. 939] were usually thrown to the winds. He would call his own intuitions "facts" or "data", while calling his rivals' intuitions mere "factual claims" or "interpretations" [69, p. 101]. When he (atypically) allowed that experimental tests of other people's intuitions could be relevant, this was because—so he reported, or predicted—they would align with his own [60, pp. 19 and 74].

Given Chomsky's sustained refusal to heed Labov's warnings, myth-item 2 is highly questionable. No matter what specific claims about syntax are in question, the general point about how (not) to discover linguistic facts suffices to undermine them. (Undermine, not disprove: to question myth-2 is to suggest some reason to doubt whether his theory is correct, not to try to show that in fact it isn't.)

It's relevant, here, that Chomsky's list [29, p. 1096] of alternative types of evidence for discovering linguistic facts doesn't include linguistic corpora—that is, collections of naturally-occurring utterances, culled from a variety of spoken and published sources (mentioned in my book at pp. 624, 681–683, and 1449). True, he says his list isn't "exhaustive"; and he even adds "in fact any source" at the end. But it's significant that he fails to mention corpora, because these are the most plausible alternative to intuition as a way of discovering "empirical facts" about language.

Until the early 1960s it was understandable that linguists put their trust in intuitive judgments (whether or not they also heeded Harris's or Labov's warnings), because there were no computers capable of analysing large corpora—and no such corpora available to be analysed. Indeed, in a discussion in 1958 in which structuralist linguists were recommending the use of corpora and Chomsky was denying their relevance, someone said: "Well, if I had three months of speech on tape, it would have to be carded and sorted by an IBM machine of pretty big proportions, before I could go to the corpus to answer any questions. I think such a program is unlikely in a practical world" [60, p. 78].

Chomsky wasn't worried about the practicalities, for he dismissed the very idea of relying on corpora. In this discussion, and in subsequent years, he insisted repeatedly that knowledge of linguistic facts was available from the intuitions of the native speaker.

Moreover, his own intuitions were treated as paramount: "The trouble with using a corpus is that some authors do not write the English language. Veblen, for example, speaks of 'performing leisure', and the verb *perform* cannot take such an object [i.e. a mass-word]" [60, p. 28]. In other words, any actual usage recorded in a corpus can be ignored if it doesn't fit Chomsky's theory. And this applies not just to the false starts and unintentional mistakes common in spontaneous speech, or the syntactic 'solecisms' committed by the unschooled, but even to the carefully considered constructions of experienced native-speaker writers such as Thorstein Veblen. In defence of his remark about the grammar of *perform*, Chomsky said: "How do I know [if I haven't used a corpus]? Because I am a native speaker of the English language" (p. 29). Veblen, seemingly, wasn't. (So much for the claim that "I've never hinted at the crazed belief [i.e. that my own intuitions were paramount] Boden attributes to the Chomsky of her imagination, which is why she cites nothing": p. 1096.)

Today, there's even more reason to doubt the scientific usefulness of linguistic intuitions. Huge corpora, and the "IBM machines" to deal with them, do now exist: the British National Corpus, for example (100 million words, including 10 million of speech). These enable linguists to make objective (statistical) judgments about the usage of certain word-patterns, even of constructions that occur very rarely indeed (currently, frequencies as low as 1 in 131,302 words [109]). Admittedly, corpora aren't unproblematic. Besides the obvious danger of skewed sampling (of which there are many possible varieties), there may be theoretical problems too: some corpora aren't 'raw', but are tagged by means of syntactic labels, about which there could be principled disagreement of various kinds. Nevertheless, these utterance-collections constitute a source of relatively concrete evidence about language. (Chomskyans use corpora when they have to: in studying infants' language or dead languages, for example. But when native-speaker intuitions are available, they rely on them.)

In short, the notion that the "empirical facts" of a science (sic) of linguistics can be culled from personal intuitions is now even shakier than it was when Harris and Labov questioned it many years ago. Most linguists today would say that it's bizarre. In that—very general—respect, myth-item 2 is false.

A professional linguist might add that Chomsky's more *specific* claims about syntax are mistaken, too. I myself would not—not because I accept them, but because I am agnostic about them. My references to his A-over-A principle, for instance, were given merely to illustrate the nature of his claims, and other people's counter-claims, about the content of universal grammar (9.vii.d). Nor did I discuss any individual hypotheses within his principles and parameters, or P&P, theory (see below). So Chomsky's constantly reiterated complaint that I have no interest in the empirical facts about language meets its mark if interpreted at *that level of specificity*. As a non-linguist, I'm not concerned with the theoretical details.

More to the point, as a cognitive scientist I would be interested in them only if they were relevant to psychological studies of language—and/or to NLP, as the rival Generalized Phrase Structure Grammar, or GPSG [53], is (9.ix.d–f). Chomsky's early theories, set out in SS, were psychologically relevant—or anyway, they seemed to be. His "derivational theory of

complexity” [82] attributed psychological reality to the specific grammatical transformations defined in SS, and prompted many interesting experimental studies before being abandoned. But that is no longer true. Psycholinguists today no longer hang on Chomsky’s every word for theoretical/experimental inspiration, and cognitive scientists in general rarely mention his name.

That point could jump us straight from myth-item 2 to myth-item 10—and to Chomsky’s complaint about my describing linguistics as “eclipsed”. However, I’ll postpone that discussion until later. First, I’ll consider Chomsky’s comments on nativism—which is to say, myth-items 3 and 4. These concern whether the pro-innateness arguments offered by Chomsky in around 1960 were “convincing”, and whether the empirical evidence amassed since then has put them “beyond doubt”.

Much as my chapter didn’t aim to prove that Chomsky’s grammatical theory is mistaken (although it did make highly favourable comments about the rival GPSG theory), so it didn’t seek to prove that linguistic nativism is mistaken. The best critique of nativism that I know of is the admirably clear attack written by a Sussex colleague, the linguist Geoffrey Sampson [108]. But despite Sampson’s careful, and often persuasive, arguments, I remain open-minded on the point—and I said as much on p. 594.

However, Chomsky claims in his review that to have an open mind here is absurd. It is equivalent, he says, to seeing his granddaughter’s achievement in learning language—which her pet kitten, a chimp, or a songbird cannot do—as “a miracle” (p. 1095).

That argument (like his reliance on intuitions) isn’t new. In 2000, for instance, he opined that “To say that ‘language is not innate’ is to say there is no difference between my granddaughter, a rock and a rabbit ... So people who are proposing that there is something debatable about the assumption that language is innate are just confused. So deeply confused that there is no way of answering their arguments. [Except, perhaps, to accuse them of belief in miracles?]” [30, p. 50]. Moreover—and again, like his reliance on intuitions—it has already been roundly refuted. (Refuted, not just rebutted.)

For example, Paul Postal has pointed out that “Since rocks and rabbits lack general human attributes of *every sort*, any failure on their part to learn languages ... would, necessarily, entirely fail to distinguish the hypotheses at issue” [99, p. ix]. These are (in my words, not Postal’s): (a) that language learning depends only on general aspects of human intelligence (some of which may be unique to *Homo sapiens*), and (b) that it depends also on inborn language-specific knowledge—for instance, on expectations about the abstract form of human languages in general: the so-called Universal Grammar (UG). Miracles, in other words, have nothing whatever to do with it.

Notice that there is no third hypothesis here, suggesting that language acquisition can be “fully explained by experience”. The notion that there is *no* innate preparation for language, that the newborn mind/brain is a *tabula rasa*, is absurd. Chomsky was right about that. Quite apart from our neuroscientific knowledge, *pure* induction, with no prior guidance whatever about what patterns to look out for, is impossible—as argued in Chapter 8.vi.f. (The statistical ADIOS algorithm [44] can induce both context-dependent and context-free grammars without being given any grammatical cues; but it has to presuppose that the input sequence contains partially overlapping strings at multiple levels of organization: 9.ix.g.)

Postal’s response wasn’t new either. In essence, Willard Quine [100] had said the same thing: “[The behaviorist is] knowingly and cheerfully up to his neck in innate mechanisms of learning-readiness ... [and] unquestionably much additional innate structure is needed, too, to account for language learning.” Empiricism (as opposed to rationalism), he said, “sees nothing uncongenial in the appeal to innate dispositions to overt behavior, innate readiness for language-learning. What would be interesting and valuable to find out, rather, is just what these endowments are in fact like in detail”. Those details might turn out to confirm either one of the hypotheses (a) and (b) listed above.

Chomsky remarks that Quine, in this paper, retracted his previous methodological critique and stated that “generative grammar is what mainly distinguishes language from subhuman communication systems”. He takes this to be an acceptance of his own form of linguistic nativism, i.e. a version of hypothesis (b).

The quotation is correct, but the interpretation isn’t. To say that language is characterized by “generative grammar” is ambiguous. It may mean merely that language has a hierarchical structure wherein grammatical sentences can be (endlessly) generated by a finite set of syntactic rules—perhaps those specified by Chomsky. Or it may mean, in addition, that language depends in part on inherited language-specific mechanisms that determine the abstract structure (UG) of all human tongues. The former interpretation could be true even if the latter were false. There’s no reason to think that Quine accepted the second, stronger, interpretation. At most, he allowed that it was an empirical possibility, whose investigation would be “interesting and valuable”.

In brief, myth-item 3 was shown to be false many years ago: Chomsky’s early arguments weren’t “convincing”. (This is compatible, of course, with his empirical claims being true.)

As for myth-item 4, that’s a mixed bag. Research on children’s language acquisition, and on what Quine called “subhuman communication systems”, was hugely stimulated by Chomsky’s early work (see 7.vi passim). That’s just one of the many ways in which he has had a beneficial influence on cognitive science. Research on other species has greatly increased our understanding of animal communication—for example, in insects, parrots, dolphins, and apes. But, despite various energetic attempts to show the contrary, it appears that language—broadly: a communication system having a generative grammar (in the first interpretation distinguished above)—is indeed confined to *Homo sapiens*, as Chomsky claimed. Moreover, various aspects of our oral and respiratory anatomy seem to have evolved as adaptations enabling speech. Or rather, they have evolved, and they enable speech: whether they are adaptations specifically for language depends on the choice between hypotheses (a) and (b), above [74,75].

On the other hand, and as I remarked in response to Feldman, connectionist models of the acquisition of the past tense (see 12.vi.e and 12.viii.e) have suggested that grammatical structures can be learnt without any reliance on internal syntactic rules, whether innate or not [95–97,105]. In particular, the transitory over-generalizations (such as *mouses*, *goed* instead of *mice*, *went*) regarded by Chomsky as explicable only in terms of rules within the infant's mind occur, for purely statistical reasons, in PDP networks. Chomskyan linguists have challenged this research, pointing out (among other things) that the behavior of the networks doesn't entirely match that of the young child [94]. My own view is that the question remains open: Chomsky's claim that we employ internal rules of grammar hasn't been conclusively refuted.

Two other things would really strengthen myth-item 4. First, if Chomsky's abstract theory of universal grammar were shown by linguists to fit all known languages. As he admits, that's not yet the case. (Some linguists argue that it will never be the case: either there is no universal grammar, or if there is then it's not Chomskyan.) Second, if non-linguists were to discover that the inborn psychological and/or neurological mechanisms enabling language acquisition include some which are clearly language-specific.

No such mechanisms have yet been found. Particular aspects of our oral/respiratory anatomy may be exploited by language although initially evolved for other purposes (see above). Similarly, mirror neurons are thought by some people to be crucial in the development of language [2], but they function in respect to motor actions in general. That is, they may support hypothesis (a), but not hypothesis (b). Moreover, they are found in some nonhuman species, so can't explain why only human beings acquire language. And some cognitive scientists argue that non-communicative (internal) "generalised languages", with structural variability and compositional semantics, are needed even for representing spatial structures and planning movements in space; already present in many animal species, these might be the evolutionary base of human (communicative) language [115].

Future cognitive neuroscience may discover language-specific mechanisms. And these may, perhaps, include mechanisms underlying a Chomskyan UG, an abstract structure common to all languages. At present, however, these questions remain open.

As the latter points suggest, part of the empirical evidence relevant to myth-item 4 might come from evolutionary psychology/neuroscience (7.vi.d–e, 8.v passim). In his review (p. 1101), Chomsky takes me to task for saying that he claimed there could be no evolutionary explanation of language. However, I stand by what I said, for I did not misrepresent him.

While he mentions my quoting him on "emergence", he fails to mention that I also quoted this: "It is perfectly safe to attribute this development [of human minds and language] to 'natural selection', so long as we realize that there is no substance to this assertion, that it amounts to nothing more than a belief that there is some naturalistic explanation for these phenomena" [27, p. 83].

He also fails to mention that I cited a more recent passage, written in 1999 [28], in which he tentatively ("*might*", "*Maybe*") suggests the possibility of a spontaneous self-organization such as was described by Turing. (Turing's "evo-devo" influence, triumphantly mentioned by Chomsky as though I'd never heard of it, was discussed at length in Chapter 15.iv.) To understand the *possibility* of emergence and/or self-organization is by no means to understand how a particular case actually happened. So even if the evolution of language is no longer "a total mystery" (as Chomsky put it in 1986), since it no longer appears utterly unintelligible, he seems still to be pessimistic about our ever specifying the explanation. (For what it's worth, I share his pessimism here.)

The upshot of these remarks is that Chomsky's linguistic nativism—positing innate dispositions that predispose the baby to some abstract structure that's universal to all languages—was dubious when it was first put forward, and remains so today. (The well-known Chomskyan Steven Pinker has conceded that "UG has been poorly defended and documented in the linguistics literature" [92].) That's true even though the current (epigenetic) understanding of nativism, mentioned above in reply to both Thagard and Feldman, is more nuanced than the binary 'nature or nurture?' divide favoured in Chomsky's youth. In other words, and despite Chomsky's claim that anyone who doubts them is "just confused", myth-items 3 and 4 are both false.

Let's turn now to a few of Chomsky's other complaints. (Only a few: his nine pages of abuse contain far too many for individual attention.) First, concerning my discussion of item-5 of the myth: that his early work was wholly original. This attracted the charge that I made "energetic efforts (as always, without evidence) to show that [generative grammar] was all borrowed from the prevailing structuralist approaches" [29, p. 1098].

What nonsense! I pointed out some very broad similarities between Chomsky's early work and that of Otto Jespersen, citing Jespersen at length in so doing. In addition, I outlined some closer similarities between Chomsky's work and that of his teacher Harris—again, cited carefully (9.v.c–d). But I also pointed out crucial differences between Chomsky and Harris (9.v.e), and described Chomsky's generative grammar as "largely novel" (p. 595), and so new as to be "shocking" to the linguists of the time (p. 629). (That word "shocking" was of course meant positively: appreciation, not denigration.)

If Chomsky doesn't regard my many specific references to Jespersen and Harris as constituting "evidence", he might prefer to consult his own address to the Linguistic Society of America in 1975 [26]. He opened by speaking in warm terms of Jespersen, even saying (p. 161): "Jespersen's own view of the matter [i.e. the possibility of a universal grammar] is subtle and complex, and I think, generally persuasive". He also said that Jespersen's work, although it had "a great deal of merit", was "perhaps, premature" (p. 166), and closed his paper by remarking that his own work "extends and advances the program that [Jespersen] outlined" (p. 196). That was the very judgment which I argued in my chapter—making it clear that the extensions and advances here were considerable.

Having re-read his paper of 1975, he might care to turn to what he said in 1958, at the first major conference wherein his theory was presented: “[My] approach to syntax ... developed directly out of the attempts of Z.S. Harris to extend methods of linguistic analysis to the analysis of the structure of discourse” [60, p. 124]. Again, a judgment for which I gave scholarly chapter and verse in my text.

But we encounter an embarrassment here. In his review, Chomsky says: “There was a tradition of something like generative grammar, later [sic] unearthed by ‘Chomskyans’, tracing from classical India to Leonard Bloomfield But there is no hint of the tradition in the work of the structuralists she mentions, for a very good reason: it was completely foreign to their approaches to language, contrary to her unsupported assertions” (p. 1098). The embarrassment arises because, in the context of his review, this contradicts what he said in both 1958 and 1975. So it appears either that Chomsky was dissembling years ago, when he acknowledged the influence of Jespersen and Harris (perhaps to curry favour with the orthodoxy?—but that was never his style), or—very much more likely—that he is now shamelessly denying the historical facts, simply in order to grind me into the dust. In short, he cannot be relied on to tell the truth.

As for what follows from acknowledging the links with Jespersen and Harris, Chomsky misrepresents me yet again. To say that his early theory shared some of the structuralists’ goals, and was even reminiscent of some of their methods, is not to say that his work was “all borrowed from” them. Creativity in general, as I’ve argued at length elsewhere [12], doesn’t spring from nothing. There will always be some associative and/or structural links to past thinking, some seeds of later ideas. To trace those links/seeds is by no means to deny the creativity of the later thinkers. Rather, it is to set them in their historical context—which is what an intellectual history is supposed to do. (So, again: no rage, no ridicule.)

Links and seeds, of course, aren’t the same as mere conceptual similarities—which may exist without there being any actual historical influence. My distinction between “predecessors and precursors” (9.ii.a) made this important point. Before applying it to Chomsky’s immediate forerunners (the structuralists), and thereby to item-5 of the myth, I applied it at some length (in 9.ii.b–c and 9.iii–iv) to a number of much earlier writers—and thereby to item-6 of the myth.

Subscribers to the *AJF* wouldn’t welcome a spate of scholarly nit-picking on these matters, so I shan’t defend my comparisons between Chomsky and Port Royal or Wilhelm von Humboldt against Chomsky’s criticisms [29, p. 1102]. Indeed, *AJF* readers might say that they simply couldn’t care less: if myth-item 6 happens to be false, so what?

Well, there are three reasons why it matters. First, anyone who makes a point of situating their own work within an ancient, and no longer fashionable, tradition must expect serious readers to consider their claim carefully. It should not simply be taken for granted, still less uncritically repeated to others—such as the readers of my book. (I didn’t claim to be a “specialist” here, by the way—a sneer occasioned by my saying that I knew from my own experience that a seventeenth-century item that Chomsky had declared to be forgotten was routinely recommended to undergraduates in the 1950s [13, p. 596]. However, one genuine specialist has buried myth-item 6 very deep indeed, saying that Chomsky’s history of linguistics is “fundamentally false from beginning to end” [1].)

More important, for cognitive science as a whole, the far-from-monolithic “rationalist” tradition is largely concerned with the notoriously tricky philosophical/psychological concept of innate ideas in general, and of linguistic nativism in particular (see 2.vi.a and 9.ii). Here, problems arise with respect not only to truth but also to meaning: *just what* were the (various) writers in question intending to say? Until one knows that, one is in no position to assess the importance of rationalism for cognitive science in general.

The third reason is especially relevant for *AJF* readers. The rationalist writers named by Chomsky said things about language that are relevant to the prospects for NLP. Humboldt, for instance, discussed the creativity of language use, its cultural (and even individual) specificity, its relation to thought (cf. the Sapir-Whorf hypothesis: 8.i.a), and the manifold difficulties of translation. Critics of NLP often describe language in similar ways. The ALPAC Report of 1964, which stalled work in machine translation (MT) for almost a generation, was one case in point (9.x.e–f and 9.xi.a).

MT, already being explored in the 1940s (and envisaged in Russia in the 1930s), is just one example showing the falsity of myth-item 7. The history of NLP in general, and of Chomsky’s deep scepticism concerning it [13, p. 673], was sketched in Chapter 9 of my book (9.x–xi). I’ll ignore it here, however, as Chomsky doesn’t address the NLP sections in his review.

With respect to myth-item 8 (regarding the fall of behaviorism), Chomsky accuses me not of denying his originality but of ignorantly exaggerating it [29, p. 1099]. In truth, the ignorance here is his, caused by his failing to follow up, or even to notice, my cross-references.

According to him, I said that cognitive scientists, himself included, had overlooked Karl Lashley’s paper on serial order in behavior [73]. Moreover, he complains that I didn’t realise that the early ethologists had had much of relevance to say. He’s mistaken on each count. I’d already discussed Lashley at length (5.iv.a and e). I’d even pointed out that his serial-order talk was converted from “a mini-sensation” at the Hixon Symposium in 1948 into “a genuine sensation” in about 1960 by Miller and—guess who!—Chomsky [13, p. 266]. Similarly, I’d outlined the work of the pioneers of ethology, including four of the names listed by Chomsky, explaining how their views differed fundamentally from the behaviorist orthodoxy (5.ii.c). (Other important figures in the demise of behaviorism included Miller, Bruner, Donald Broadbent, Richard Gregory, and—of course—Simon and Newell: see Chapter 6, *passim*.)

Chomsky is especially scathing about my rejection of myth-item 10, the belief that linguistics is as prominent in cognitive science today as it was in its first quarter-century. He has fun mocking my pithy sub-heading “Linguistics eclipsed”, asking “Since linguistics is the study of human language, it is a remarkable feat to have ‘eclipsed’ it. How was that achieved?”

Someone with his keen sense of rhetoric should know better than to take my phrase literally. But I’ll gladly answer his question (as I did in my chapter). The “eclipse” happened within cognitive science, not within academia as a whole.

It concerned not linguistics in general but Chomskyan linguistics in particular. And it was achieved by Chomsky himself, through various changes in his theory.

Chomsky's early "derivational" theory of transformations, which made psychological as well as purely linguistic claims [82], excited psycholinguists (see 6.i.e and 7.ii.a–b). They found some supporting evidence for it [6,48]. But two things happened to discourage them. On the one hand, the experimental evidence for the psychological reality of transformations started to weaken [50], and the theory was later declared defunct by some of Chomsky's closest colleagues [49]. On the other hand, Chomsky changed his theory. Specifically, SS was ousted by *Aspects of the Theory of Syntax* [23]. This introduced "standard" theory, which—by reducing the number of transformations in his grammar—took the first steps on the theoretical road I referred to as "Transformations trounced".

The psychologists at that time were dismayed, not to say aghast. As the experimentalist James Jenkins put it [65, p. 243], "Chomsky pulled the rug out from under us ... [We] were very busy trying to find the [psychological] apparatus for a theory of linguistics that at that moment was being discredited ... [By] the time we could supply the right kind of theory, the nature of what language was believed to be had changed. The whole theory was no longer appropriate. Very grim, very grim".

Grim or not, if Chomsky's new theories (standard theory was only the first of several fundamental changes) had been as psychologically relevant as his early ones, then psycholinguists and other cognitive scientists would have had to pay the price of struggling to keep up with him. (I said this on p. 667, so his accusation that I criticized him for changing his mind is absurd: see Karl Popper's [98] account of science as "conjectures and refutations", cited at various points throughout the book.) But they were not. Eventually, the major problem became the forbidding abstractness of his work, in the P&P theory and the minimalist program (MP).

Another key problem (from the late-1960s on), from the experimentalists' point of view, sprang from the competence-performance distinction (CPD). The CPD, which defined a form of psychological explanation that abstracted from the performance details, was hugely influential in the early years of cognitive science (see 7.iii.a).

It was welcomed enthusiastically by Marr, for example, who applied it in his theory of low-level vision [78]. And, as Chomsky rightly says, it soon led to "rich and important" work in developmental psychology—detailed in 7.vi.a (which disproves the slur that I omitted even "the most casual investigation of the literature" [29, p. 1101]). So it didn't instantly put an impenetrable firewall between theoretical linguistics and experimental psychology. Nevertheless, it tempted psychologists to ignore performance details that could not be fitted into the abstract linguistic theory. The characteristics of anxiety-ridden speech [36], for example, wouldn't have been studied by Chomskyans (see 7.ii.c).

As the abstraction continued to increase, the firewall became even stronger. It became very unclear just what experimental evidence, as opposed to the data available in intuitions and/or corpora, could confirm or disconfirm the linguistic theory. It was also unclear just what new lessons his later theories had to teach us about the nature of mind in general. That's why I said very little about P&P, and even less about MP.

In discussing P&P, I pointed out that it is potentially relevant to the recent revival of interest in nativism as epigenesis. As I said in my reply to Thagard, this concept is prompting exciting work in developmental psychology, neuroscience, and biology, and has also influenced A-Life—and even robotics (7.vi.g, 14.ix.c–d, 15.viii.a–b). Piaget had been expounding epigenesis for many years, with scant support from Chomsky [91] (5.ii.c). But only relatively recently has the concept surfaced in cognitive science. Even so, the possible relevance of P&P is rarely, if ever, remarked.

The details of P&P (assuming that they can be confirmed by linguists) could be interesting to cognitive scientists in other ways, too. For instance, one Chomskyan has tried to couch empirical evidence about the acquisition of different languages in P&P terms, including suggestions as to why this or that parameter is chosen in a particular case [56]. Another has applied P&P to historical linguistics, seeking to explain why certain patterns of grammatical change occur repeatedly [101].

Because of this untapped potential, I had intended to give illustrations of one or two suggested parameters in my chapter. But I found, when drafting, that this would take up more space than would be warranted. What's more, I wasn't at all sure that I could understand these highly abstract matters well enough to do so. (For helpful recent discussions, see [4,133].) Perhaps other non-linguists have the same problem. Whatever the reason, the fact remains that there is no mainstream cognitive-science interest in P&P.

As for minimalism, this too has been pretty much ignored by non-linguists, newspaper articles notwithstanding (see above). In principle, MP would be of greater interest than P&P to *AIJ* readers, because it aims to show how general considerations, such as computational efficiency and optimal search, could lead to UG being like *this* rather than *that* [102]. In the terms used above, it seeks to minimize (sic) the domain-specific UG component, so as to push nativism as far as possible from hypothesis (b) towards hypothesis (a). But in practice, it is far from being a shopping-list for work in computer science, NLP, or psycholinguistics.

Had Chapter 9 been intended as a history of Chomskyan linguistics as such, my very brief treatment of P&P and MP would have been a serious fault. (Similarly, a history of cognitive psychology as such would have discussed reaction-time techniques, and Bayes' theorem too: see my response to Thagard, above.) But it was concerned, rather, with how Chomsky's various theories have influenced cognitive science. In these two cases, they haven't.

The falsity of myth-item 10 has been reluctantly acknowledged by the Chomskyan Ian Roberts [102]: "Unfortunately, this [i.e. my phrase 'Linguistics eclipsed'] is a largely correct observation". It's conceded also by Chomsky's ex-pupil Ray Jackendoff, who has allowed that the discipline is now "arguably far on the periphery of the action in cognitive science" [64, p. 651]. Admittedly, types of linguistics that aren't focussed on syntax remain, and are much further in from the "periphery".

I've already granted, in reply to both Thagard and Feldman, that my discussion of cognitive linguistics in Chapter 9 was far too brief (even though I explored some relevant theories in other chapters: 7.iii.d, 8.vi.e, 12.x.g). Linguistics may regain its place in cognitive science—if not as a *root* of theorizing in the other disciplines, at least as an equal, and collaborative, partner. If that happens, however, it will be a very different enterprise from that engaged in by Chomsky. In short, “eclipsed” seems about right.

And yet, and yet If Chomsky's name is rarely mentioned today by non-linguists (except perhaps by those working on nativism/evolution), and if his current theories are addressed even more rarely, his general contribution hasn't been eclipsed. To the contrary, his early lessons have been well learnt. Cognitive scientists no longer doubt the importance of the themes that Chomsky highlighted—even if a minority (for example, proponents of situated robotics and/or dynamical systems) are highly sceptical about some of them. Indeed, these themes are taken for granted, and not thought of as specifically Chomskyan. They include the definition of generative grammars (for many kinds of behavior); comparisons between the computational power of different systems; the existence and nature of mental representations; hierarchical structure in minds and behavior; the CPD; formal description/explanation in psychology and the philosophy of mind; and the plausibility of some form of nativism/epigenesis in language (and in other areas, too).

In short, cognitive science owes a huge intellectual debt to Chomsky, even though he is less of a name to conjure with now than in the 1960s/1970s. That's what my Chapter 9 was saying—so the urologist's description, albeit highly amusing, wasn't well-aimed. (Yes, the pun is intended!)

Finally, a word about the highly unpleasant tone of Chomsky's piece—which led the e-mailing MD to come up with his memorable description in the first place.

Among the countless personal sneers scattered within it is one that I find very funny. I'm minded to get out my embroidery silks, to sew it as a richly-colored sampler for my study wall. He says (p. 1096): “But none of this matters. The phenomenologist has spoken.” His scorn here is directed at me, although his word (“phenomenologist”) is possibly aimed also at a Finnish linguist whom I'd quoted. The barb is laughable: as my remarks (above) about Continental philosophy show, the last approach that I'd follow uncritically is phenomenology. Well, perhaps not the *very* last: “post-modernist” would have been even funnier.

The other insults in Chomsky's piece are less amusing. They don't merit individual attention, and nor do they merit publication in a serious journal. But such discourtesies have been part of his intellectual armoury for years. The contemptuous tone, the systematic misrepresentations, and the many falsehoods that characterize his review are all typical.

Others, too, have received the catch-all accusation (p. 1094) that “virtually every reference to me . . . is fanciful”. Compare, for instance, “I see no reason to try to trace the various confusions that [the philosopher Gilbert] Harman develops, none of which have any relation to the views I actually hold” [24, p. 154]. Or consider this, aimed at the (then) generative semanticist Lakoff: “[he has] thoroughly misunderstood the references he cites . . . presents a hopelessly garbled version . . . [and] has discussed views that do not exist on issues that have not been raised, confused beyond recognition the issues that have been raised and severely distorted the contents of virtually every source he cites” [25].

No dissident can escape his arrows. Even being a Finn is enough to prompt a jeer from the master. (And possibly a libel: saying that the Chomsky quotation I cited from my Finnish source [63] is “presumably invented”, he cannily leaves it ambiguous as to which of us is being accused of such rampant dishonesty.)

This intellectually shoddy treatment of anyone who dares to disagree with him is not merely non-Legendary, but positively harmful. It has contaminated the discipline—hence the doubt about myth-item 9 (that he reawakened and strengthened linguistics as a whole). His combative rhetorical stance has helped to lower the standing of other sub-areas, such as sociolinguistics or comparative philology, since he often implies that anything he isn't interested in is trivial (see below). And within the study of syntax, it has raised the emotional temperature considerably. His followers are tempted to imitate. (Fortunately they don't always do so: my book received an entirely courteous review from one of them [102]). And his opponents are often so enraged as to reply in kind.

That's largely why non-Chomskyans are passionate in their opposition (and why I, as an outsider to the field, didn't feel a need to endorse the vitriol in their remarks). And it's why I didn't have to do much digging to “unearth” (Chomsky's word) the highly-charged critiques quoted at the opening of his review. Anyone who doubts that has only to look at the aptly-named survey of *The Linguistic Wars* [57], or at Postal's blistering critique of Chomsky's “often disturbingly unserious, indeed irresponsible” style of argument, with its many “outrageous” and “grotesquely untrue” remarks [99].

Other fields, of course, have their feuds and schisms. Several of these are described in my book—including five intellectual scandals relating to AI: MT versus ALPAC, the Dreyfus and Weizenbaum affairs, the attack on perceptrons, and the Lighthill Report. My AI chapters therefore mentioned some “juicy gossip” about conflicts, as Feldman puts it. So did the sections dealing with disputes about the Science Wars (1.iii.b), the analytic/Continental divide in philosophy (1.ii.c–d, 11.ii, 16.vi–viii), and the proper nature of psychology—empirical or hermeneutic (6.i.d, 6.ii.d). The fight between cognitive and hermeneutic anthropologists was especially nasty (8.i.d, 8.ii.a–c). Disappointing though this must be for Thumper's mother, cognitive science is not all sweetness and light.

But no other discipline within it is so notoriously ill-tempered that I felt bound to open my chapter with a health warning (“beware of the passions that swirl under any discussion of Chomsky”: p. 591) and a minatory section headed “A non-pacific ocean” (7.i.b). In short, the disciplinary situation in linguistics is dire. It goes way beyond the robust argument—and yes, the catty infighting—endemic in other areas of intellectual life. And, although I didn't explicitly say so in my book (because I

wasn't engaged in a pissing contest), it is Chomsky's habitual manner of treating his critics which is the key to this sorry state of affairs.

Roberts [102] agrees about the direness, but blames the anti-Chomskyans. He says that the “meme” of Chomsky as an intellectual dictator “performs a useful rhetorical function for Chomsky's critics: it poisons the well for his defenders. It means, for example, that all of the points I [Roberts] am trying to make in this article can be safely ignored, since I can be viewed as either a terrorised apparatchik or as a mindless acolyte”. That may well be true—and if so, it's highly regrettable. But *why* did this lamentable state-of-affairs come about? Roberts suggests (p.c.) that it is a generational thing, a hangover from the “linguistic wars” of the 1970s/1980s, and that the youngsters will be less obstreperous. I hope he's right. But even if he is, we're still suffering the direness now. And it's clear that Chomsky's own lack of collegiality and good manners is largely responsible.

Even as a young man, he could use both invective and ridicule as weapons. His review of Skinner [20] achieved its notoriety partly for that reason. Words such as “pointless”, “confused”, “gross”, “absurd”, “delusion”, “arbitrary”, “useless”, and “empty” leapt out from the pages, and Skinnerian art-appreciation was said to be best conveyed by shrieking “Beautiful!” repeatedly, without ever pausing for breath. (I don't say this as a criticism: in the context of his careful and intellectually powerful critique of Skinner's theory of language, these rhetorical flourishes were acceptable—indeed, enjoyable.)

But at that time, fifty years ago, he could practice courtesy too. In 1958 he was given a rough passage by the about-to-be-ousted structuralist linguists, at a conference held soon after the publication of *SS* [60]. Chomsky's own presentation was inoffensive enough [21], but the post-talk discussions that appeared in the *Proceedings* were robust. And some remarks suggest that the atmosphere wasn't always congenial: speakers complained that “At this point communication seems to have failed” (174), or referred to “the danger that communication would break down completely” (p. 182) and the need that “if we are going to live together, we must do our best to keep our wars strictly intellectual” (p. 177). The organizer, Archibald Hill, closed the meeting diplomatically by saying: “I am afraid we are not quite fair to each other, since I do not believe that any of us makes a serious attempt to work it out as the other fellow has done it. We should, and we should have respect for each other, and study what each of us has done” (p. 186). In light of that sage remark, I suspect that many heated discourtesies had been exchanged in the smoke-filled rooms.

For all that, the published discussions were scrupulously polite—Chomsky's included. At one point (p. 161), he even said: “[If] I have given the impression that anything that I am not personally interested in, is trivial, I apologize.”

Anybody finding those two little words in one of Chomsky's more recent publications might need smelling-salts to help them recover from the shock. Despite his unparalleled professional success since those early days, he has long forsaken courtesy for insults—and even calumnies. Dissenters regularly receive abuse, rather than critique.

(That's assuming that he deigns to reply to them at all. His insistence that he had to be asked “several times” to write his review for the *AJ* is consistent with a recent report—with ample evidence provided—that Chomskyans use “sulking as an intellectual strategy” [109]. They typically refuse to give their opponents “the oxygen of publicity” by citing them, or by agreeing to speak from the same platform. One prominent Chomskyan, namely Pinker [93, p. 171f.], even failed to cite an opponent [108, p. 128] whose argument he was specifically rebutting, and whose—highly unusual—examples he was borrowing.)

A remarkable case of unrestrained abuse on Chomsky's part was noted by William Bright, an expert on indigenous American languages and the key founder of sociolinguistics. As editor of *Language* for the previous twenty years, Bright had written to Chomsky in 1984 urging him to submit a paper. It was a pity, he felt, that the discipline's leading journal hadn't published anything by its leading theorist for many years. Indeed, MIT-trained linguists in general had virtually stopped sending items to *Language*, favouring two newer journals instead (one founded only in 1983).

Chomsky's reply was not your usual polite brush-off: *Sorry: too busy, too many deadlines ...*. To Bright's amazement, he wrote (alongside other abuse) that he would never consider publishing in a journal that had published “flat lies ... couched in a rhetoric of a sort that might be appropriate to some criminal, but that one is surprised to find in a scholarly journal” [61, p. 636].

Truly, the mind boggles. Compared with invective like that, his derisive “phenomenologist” is mild indeed. (Perhaps that's my loss: “criminal” would have inspired an even more delectably ridiculous sampler for my wall.) This extraordinary incident, if nothing else, suggests that a linguist colleague may have been right when he said to me recently, “To be savaged by Chomsky is a badge of honor”.

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